

A multitude of new species are described, and others already noticed have been more fully investigated and the details given. Naturally, many European species occur also in America. For these the author has mainly (as is acknowledged) made use of Mr. Buckton's yet incomplete monograph of the British species, adopting also the latter author's somewhat unscientific form of bibliographical and synonymic quotation. Some very glaring typographical errors are corrected, but only in the place where they first occur, although constantly recurring; others almost equally important are not noticed.

Zur Kenntniss der Fauna des untersten Lias in den Nordalpen. Von Dr. Neumayer. (Vienna, 1879.)

ENGLISH geologists who are interested in the study of the Infralias, will welcome this latest contribution to science by the indefatigable palæontologist of Vienna. The fossils described have been obtained principally from three localities—Pfonsjoch, in the Northern Tyrol, Breitenberg, in the Osterhorn group, and Zlambach in the Traunthale. Among the sixty-six forms here noticed, a large proportion are either identical with species which have been described in Western Europe or present such slight points of difference that Dr. Neumayer has not felt himself warranted in giving them distinct names. It is very interesting to find how close is the agreement in the general characters of the fossils of these Infralias beds in the Mediterranean province with the fauna of the strata on the same horizon in England, France, and Swabia. As in Western Europe, so in the Alpine province, we find the numerous varieties of *Ammonites (Aegoceras) angulatus* and *planorbis*, especially characterising the zone by their great abundance; while *Ostrea arietis*, *Lima punctata*, *L. gigantea*, *L. succincta*, *Modiola psilonoti*, *Myoconcha psilonoti*, and *Unicardium cardioides*, are associated with these ammonites in both areas. Besides these familiar forms there occur, however, some others which are quite unknown in Western Europe. Dr. Neumayer's monograph is illustrated with seven well-executed lithographic plates, and is a very valuable contribution to our knowledge of the Jurassic formation in the Alps.

J. W. J.

Africa Past and Present. By an Old Resident. (London: Hodder and Stoughton, 1879.)

IN "Africa Past and Present" the writer carries us back to the time when Herodotus, collecting material for his history, in the absence of written documents, travelled to Africa. Then follow chapters on enterprising Arabs, who penetrated into the interior of the country at a far distant period, and on the Portuguese early English and French discoveries. Accounts are given of the travellers who were sent out by the African Association to explore the interior of the country, prominent among whom were Mungo Park, "whose melancholy fate did not damp the ardent desire of the British public for further information concerning the interior of the great continent." Then follow descriptions of the more recent adventures and discoveries of Speke, Grant, Baker, Livingstone, and others, though the author makes no reference to the important work done by recent German explorers. The latter half of the book is devoted to the history and physical geography of the country, the author taking each division and giving topographical details of it, and speaking of its climate, resources, productions, and character, manners, and social condition of its inhabitants. The book is intended as a handbook for missionaries, merchants, travellers, and emigrants who wish for information about Africa. As such it will be useful. The book has many illustrations and a map of the country. It has also the advantage of being cheap and portable.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Ice-Crystals

I HAVE been prevented by other work from thanking your correspondents who offered explanations of the peculiar forms taken by ice-crystals upon rotten ligneous fibre.

Will you, however, allow me to say that the explanation offered is one respecting which I am very sceptical.

That explanation is that the long filaments, like spun glass, are merely the result of the internal freezing of the moisture in the substance of the wood and of the expansive force of that freezing pressing the ice thus formed through the pores of the wood.

My impression is that if this were the cause the expansive force would be sufficient to destroy the ligneous fibre altogether, and break it up. I question also whether there are any pores or tubes of the kind and size required by this theory running in the direction of the medullary rays. Lastly, as upright arborescent forms of ice-crystal are formed upon dry wood and upon other substances, which cannot possibly be due to any such cause, I am inclined to think that this particular form is determined by some other cause than the one suggested. The filaments are much too long and much too crystalline in structure to be the mere result of extrusion from an internal mass of ice.

Argyll Lodge, Kensington, February 14

ARGYLL

Koenig's Collection at the Philadelphia Exhibition

MY attention has just been drawn to the fact that a report has recently been circulated in London to the effect that the splendid collection of acoustic apparatus exhibited by Koenig, of Paris, at the Centennial Exhibition of 1876, had been retained in this country for the Stevens Institute of Technology, under promise of payment, and that nothing had been paid for it.

As regards the Stevens Institute, I have to say that the report is utterly without foundation.

We have never had one of the instruments in our charge, nor has a word ever been said about purchasing it for our use.

The collection was, in fact, removed from the Centennial building to the University of Pennsylvania at Philadelphia, which is about one hundred miles from here, where it now remains, and it has been currently reported that a gentleman in Philadelphia had presented it to the said University. As to that part of the story I know nothing, but I do know absolutely that the Stevens Institute of Technology has never had anything directly or indirectly to do with the matter.

HENRY MORTON

Stevens Institute of Technology, Hoboken, New Jersey,
February 4

"Scientific Jokes"

I DO not know who your correspondent "G. H." may be, but I should surmise from the tone of his letter that he is somewhat of a beginner in science, and that he is so proud of his acquaintance with certain elementary propositions in thermodynamics, that he is on the *qui vive* to detect in others an ignorance of them. In my opinion the fair meaning of the passage objected to, when read with its context, is that the author is drawing a parallel between temperature in heat and potential in electricity (between which there are striking analogies), and that the words to which your correspondent refers are purposely employed to prevent any one imagining (as "G. H." seems to have done) that it was intended to represent the energy of heat as the *product* of heat and temperature in the same manner as that of electricity is the product of quantity and potential. Temperature is treated as inseparable from heat and nothing more, just as potential is inseparable from electricity, and this is not an unscientific view of the matter.

The latter part of the letter relating to the theory of terrestrial magnetism, propounded by Professors Ayrton and Perry, is, I

think, still less creditable to the writer. Whatever be one's own views on the subject, the question of the tenability of the theory is still *sub judice*, and it is not becoming in "G.H." to speak so contemptuously of the author of the address for not taking the same view as he does of the merits of the controversy.

74, Onslow Gardens, S.W.

J. FLETCHER MOULTON

On the Mode of the Transverse Propagation of Light

IN NATURE, vol. xxi. p. 301, is a letter by Mr. W. M. Hicks containing some critical remarks on a paper of mine, "On a Mode of explaining the Transverse Vibrations of Light" (NATURE, vol. xxi. p. 256), which I shall be glad to notice here.

Firstly, it is, no doubt, understood that the theory proposed by me cannot be regarded as *in opposition* to any existing theory, from the simple fact that no theory or clear conception of the constitution of the ether (in regard to the mode of propagation of the transverse vibrations of light) appears really to exist. The notion of the ether resembling a "solid" or an "infinitely thin jelly," cannot, of course, be regarded otherwise than as a resource in the face of a difficulty, which, however, we think must appear to any impartial inquirer to increase rather than diminish the difficulty; and therefore the inference would seem a not unreasonable one that any *true* theory of the constitution of the ether would be something totally different from "statical" theories of this kind. As it has been one of my objects to prove, after considerable attention given to the subject, that but *one* view of the constitution of the ether is in principle conceivable (or that one solution to the problem already exhausts the limits of the conceivable), I may therefore be excused for having some confidence in the fundamental groundwork (at least) of the view adopted, and am therefore all the more ready to reply to any criticisms on the subject, though no doubt (as in the case of any theory possessing points of novelty) difficulties may be expected at first to arise that may entail considerable thought to remove them. It need not be premised that the attainment of truth is the ultimate object of all.

In the first place, in regard to the remarkable means of correcting and adjusting their own motions that atoms moving freely among each other have been proved to possess, I may at once withdraw the expression "instantly," in regard to the rate at which this self-adjustment takes place. The expression is at best a vague one, and the idea arose from the known fact of the practically instantaneous adjustment that takes place in the case of an ordinary gas. The mean velocity of the ether atoms would, of course, be necessarily equal to that of light, and all that is essential is that the adjustment should be rapid enough to maintain adequately the equilibrium of the ether.

In regard to the second difficulty mentioned; I do not see that the fact of some of the atoms of ether moving at a greater or less velocity than the *mean* velocity (which is equal to that of light) should put a difficulty in the way of accounting for the regularity of the waves of light. For it has been proved in connection with the kinetic theory that the number of atoms whose velocities differ by any great proportion from the mean velocity is relatively very small. These atoms would no doubt distribute the energy irregularly over the beam of light, but the total effect would in this way neutralise itself. The great majority of the atoms would still be moving at the mean velocity and distributing the energy in regular waves, and producing that sequence of energy that we call light. I may note that in a paper on "The Mode of the Propagation of Sound on the Basis of the Kinetic Theory of Gases," published by me in the *Phil. Mag.* for June, 1877, and where a mathematical determination of the velocity of the wave was appended by the late Prof. Clerk Maxwell—the same considerations regarding the varying velocities of the atoms would be involved as above; and yet we know that as a fact the sequence of the waves of sound is in perfect regularity.

In reference to the third difficulty mentioned by Mr. W. M. Hicks, regarding the explanation of refraction and reflection. This leads me more strongly to return to a detail in regard to the constitution of the ether I had before adopted, but had not fully grounded, probably from the absence of the requisite encouragement to devote an adequate amount of thought to the subject. I quote the following in substance from a paper already written. I am led to regard the ether atoms as of *two* grades of dimensions. Of course there is no *a priori* reason why they should be all of one size, and the fact of their being of two sizes does not alter the principle of the theory in the least. They

may therefore be assumed, if facts require it, to be of two grades of dimensions. The one set of atom (specially concerned in the effects of gravity) are to be considered as enormously smaller than the atoms propagating light, and consequently their velocity (which will adjust itself automatically in the inverse ratio of the square root of their mass) very much greater. It might perfectly well be assumed (for example), that the mass of the atoms producing gravity is such that their velocity equals, say 10,000 times the velocity of light. I would just remark, in connection with this, that the expression "wonderful" sometimes applied to the velocity of light is, I think, to be deprecated. I would submit that there is nothing really "wonderful" in any velocity, because, however great a velocity is, it is always indefinitely small compared with that which it might be conceived to be, as one has in strict logic no power to limit arbitrarily the conceptions in this respect. If, therefore, there be reason for inferring a certain velocity to exist (no consequence what its value), it seems to me there is no ground for assuming it to be "wonderful." If a body or atom moves in free space without obstruction, there is nothing to curb its velocity, and its energy may even become immeasurably small at this velocity, provided the atom itself be small; and, in the same way, we have nothing to limit our conceptions as to the smallness of atoms. There can be no difficulty whatever in these conceptions, as mechanical principles are admittedly independent of *scale*, and therefore there is nothing mysterious whatever in the subject. The real mystery surely attaches to the spiritualistic assumptions about "forces" which spoil the interest of physical inquiries, and have involved that magnificent physical agent, the ether, in such a labyrinth of spurious mystery as to repel the inquirer. I cannot avoid the inference that any one who reflects seriously and impartially on the subject, will be disposed to admit that there really cannot be *two* methods in physical science, but only *one* method (the dynamical), the so-called "statical" speculations about "forces" leading nowhere. It has been proved again and again in connection with science that the so-called spiritualistic "method" is utterly barren, and only involves one in an inextricable maze of speculation from which there is no escape. I have thought these few remarks necessary in view of the special subject with which I am dealing.

It will be observed that the whole of the dynamical effects above referred to are automatic. The correction of the motion of the atoms so as to move in the right way to produce gravity and light is automatic; the adjustment of the relative velocities of the atoms between the two sets is automatic, or we make no arbitrary postulate at all. The effect of an adequate velocity for the smaller set of atoms would necessarily (from well known dynamical principles) cause them to oppose no measurable resistance to the molecules of gross matter vibrating in them, and consequently they could take no measurable part in the propagation of the energy of light. They would, on the other hand, produce an enormous pressure (adequate for gravity) on the molecules of gross matter—the pressure being as the *square* of the velocity.¹

The main reasons for assuming that the atoms producing gravity and those producing light are separate, are first the great pressure requisite for gravity, and the consequent necessity for an adequately high velocity to produce this pressure, and secondly (as Mr. W. M. Hicks points out), there would appear to be a difficulty in explaining the reflection of light from some bodies, and also the phenomena of refraction, if we assumed the atoms propagating light to pass through all bodies with perfect facility, as is necessary in the case of the atoms which produce gravity. As this letter has already grown to some length, I will at present confine myself to this inference, reserving some ideas relative to polarisation (in connection with the present theory) to a subsequent letter.

To prevent misconception, the fact may be cited that the above kinetic theory of the ether does not represent an *emission* theory of light. The motion of translation (which the ether atoms would possess if there were no light) merely serves as the carrier of the energy impressed upon them by the vibrations of the molecules of gross matter. On the other hand, the fact of the theory resembling (in the translation of the atoms) *one* of the ideas of Newton may possibly be regarded as rather a recommendation than otherwise. If, however (as I have certainly set

¹ If observation shows light to suffer no (sensible) diminution of velocity at reflection, it would follow that the luminiferous atoms do not suffer a (sensible) diminution of their transitory motion; rebound from gross matter, and consequently these particular atoms could not be appreciably concerned in the effects of gravity.